

Haskins (E. B.)

# THERAPEUTIC CULTIVATION;

## ITS ERRORS AND ITS REFORMATION;

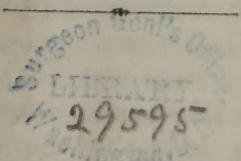
### AN ADDRESS,

DELIVERED TO THE

# Tennessee Medical Society,

APRIL 7, 1857.

BY E. B. HASKINS, M. D.,  
P R E S I D E N T.



NASHVILLE:

CAMERON & FALL, BOOK AND JOB PRINTERS, CORNER COLLEGE AND UNION STREETS.

1857.

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## THERAPEUTIC CULTIVATION.

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GENTLEMEN:—For no inconsiderable time it has been the complaint of medical philosophers and philanthropists, that Therapeutics, or the art of medicine, has not advanced as rapidly as the science of our profession. In other words, that notwithstanding we have made great progress in the knowledge of anatomy, physiology, pathology, etc., yet in the treatment of disease, we have made, comparatively, but little improvement upon the methods of the ancients—that we have been more apt in detecting nature's movements than expert in controlling them—that we have become learned faster than we have grown skillful. Why is this so? How can it be remedied? These are questions that deeply interest the practitioner everywhere, and their solution merits his most anxious and profound consideration.

The *object* then of the address which the usage of this Society imposes upon me on the present occasion, is to enquire into the causes of the comparative tardiness of therapeutic advancement, and the means best calculated to remove them—and therefore its *subject* is

### THERAPEUTIC CULTIVATION, ITS ERRORS AND ITS REFORMATION.

Although it will be seen in the course of our enquiry, that some of the most potent causes which operate to retard therapeutic progress, are inherent, and therefore unavoidable, and consequently any unfavorable contrast against the cultivators of this branch of medicine must be more apparent than real; yet I will not here argue their defence,—I will not stop to enquire how far they may or may not be deserving of

censure. It is better to acknowledge our faults than to proclaim our virtues. Reformation can be hoped for only where a sense of imperfection exists. I presume it may be granted, without injustice to any, that there is yet room for reformation in every department of medical learning. Without further preliminary then, I proceed to inquire into

THE CAUSES OF THE COMPARATIVE TARDINESS OF THERAPEUTIC ADVANCEMENT.

1. The first cause to which I invite your attention, *consist in the wide extent of scientific field which the art of Therapeutics covers—the complication and widely dissimilar nature of the facts and principles involved—requiring such a vast amount of labor, as well as varied and multitudinous qualifications, both natural and acquired, for successful observation, experiment and deduction.*

The anatomist, physiologist, pathologist, histologist, chemist, &c., confines his researches mostly to his specialty, whilst the practitioner or therapist must master, and keep himself familiar with the daily progress of all of those specialties, to enable him to occupy with success his own field of labor. Besides this laborious educational preparation, his qualifications for practical observation, thought and action, must possess a strength and versatility parallel with the magnitude and variety of the sciences that forms the basis of his art. He must possess the tact and talent requisite to analyze and determine the nature of morbid humors—to resolve by microscopic examination the elements of diseased tissues—he must have the faculty of defining the shades of sound emanating from a diseased lung or heart, as well as be endowed with that common sense power of mind that grasps and comprehends, as by intuition, the physiognomy of disease as portrayed in the general aspect of the subject. But with all of these qualifications must his labors be inefficient, unless he possess a logical mind—that enables him to analyze the *ensemble* of morbid phenomena; determine their relative value; trace the causal connection of his remedies and the alteration of morbid processes; and protect himself against sources of fallacy in the different steps of his investigations.

But these qualifications, requisite as they are to the successful

cultivation of Therapeutics, could not supply the want of a knowledge of the power and meaning of language, with that happily tempered imagination that enables one to describe with accuracy what he has witnessed—to convey by pen-painting, if I may so speak, to other minds the impressions and ideas made upon his own. But above all, must the cultivator of Therapeutics possess the moral attribute of a *love of truth*. He must not only be imbued with that sense of the importance of good faith in the cultivation of medicine common to all lovers of science, but he must comprehend and morally appreciate the peculiar nature of his own department, that renders truth and candor in its cultivation, paramount to that of all others. When a discovery is announced in physiology, chemistry, or pathology, nothing is hazarded in the attempt to verify or disprove it but the loss of time, and perhaps the lives of some of the inferior animals; but when a new therapeutic agent is proclaimed, human life may have to pay the penalty of an error. When a Bernard announces to the world that the liver is the organ in which the glucose of the organism is elaborated, or a Brown-Sequard declares to have discovered that it is mainly the central grey substance of the spinal marrow that conveys sensitive impressions to the brain, experiments are repeated upon the lower animals by other physiologists without the hazard of evil, whether they verify or contradict the former announcement; but when it is asserted that large and repeated doses of quinine will abort typhoid fever, Epsom salts or tartar emetic will cure Asiatic cholera, or that lemon juice is a specific for acute rheumatism, hundreds of practitioners of every grade of learning and skill become experimenters,—man is made the subject of experiment, and human life and health is staked upon the issue. But this is not all: When a physiological or chemical result is announced, other experimenters verify it, or else the error is detected and science cleared of the rubbish; but when a clinical result is claimed, the occasion and the material disappear with the observation, and no one can repeat the exact experiment or re-witness the observation. Others may fail to obtain the same results under analogous circumstances, and thus, as a practical question, it may in time cease to annoy us, yet claiming to be a fact, it remains as a clog to our literature, and

throws an air of doubt and uncertainty upon the canons of our practice.

2. The second cause of the comparative slow advancement of Therapeutics, of which I propose a brief notice, is found in the *scarcity of available material for experimentation.*

Had physiology depended for its cultivation alone upon human subjects, its advancement would perhaps at this day have been behind that of Therapeutics. Facts evolved from experiments upon the lower animals have been allowed as legitimate data for solving physiological problems in man; the liver, the pancreas, the vascular and nervous systems, etc., of the dog, rabbit, and other inferior animals have been considered sufficiently identical in structure with those organs and tissues in man, to deduce from the results of experiments upon the former, the functions of the latter; hence the available material has, in all time, been even more abundant than the most active wants of science demanded. Not so, however, with therapeutics. The diseases of the lower animals do not resemble those of man—at most in so slight a degree that they afford no facilities for therapeutic cultivation. Aside from the action of some poisons and their antidotes, the inferior animals have not, and never can serve as valuable material for experimentation in the cultivation of Therapeutics.

Then as human subjects alone have furnished the material for therapeutic cultivation, and as the moral and religious sense of the profession has denied the right of experiment in the proper sense of that term, therapeutics has been dependent for her clinical facts mainly upon accidental discovery.

3. I am now brought by these remarks to a third cause which unhappily retards the progress of our art, to-wit: *The dependence of therapeutic cultivation upon the contingency of popular choice of the cultivators.*

Men are unequally endowed with mental powers, adaptations and capabilities—all are not prepared by nature, education and taste for the same pursuits—hence the importance of a free and untrammeled exercise of the will in the selection and full prosecution of that pursuit dictated by the taste, and a conscious mental adaptation. In every branch of medical pursuit this state of things exists, except that of therapeutics. The ardent

youth who has a taste for experimental physiology, anatomy or chemistry, and feels himself competent to the prosecution of the task, has only to prepare himself, and all nature stands ready to furnish him with materials,—no social imperfection stands in the way of his progress, or robs science of the rich results of his genius. Not so, however, with him who proposes to enter the field of therapeutic labors—however well trained may be his mind, and however well adapted by nature and by cultivation to the observation of disease and its progress under treatment, unless he has those qualities called popular—unless he has the faculty of captivating the public mind, he has no chance of bringing his powers into active requisition. And when it is remembered how few possess the requisite qualifications for trustworthy clinical observation, and how important it is to embrace the largest possible amount of experience to deduce valuable practical results, it becomes at once apparent how great must be the waste of material in the field of therapeutic labors. The evil of this waste too, becomes the more glaring when we record the fact, that in order to make clinical observations of the highest degree valuable in therapeutic deductions, they should cover as little space and time as possible. In other words, that in order to make clinical experience of the utmost value, it must embrace the same circumstances, (or as near the same as possible,) both of season and of locality.

To make myself better understood under this head, I beg leave to illustrate by example: Let us suppose that in this beautiful city there are twenty-five practitioners, out of the large number located here, who have, in vulgar parlance, a living practice—twenty-five who the good citizens have declared by their patronage to be competent to cultivate this highly important branch of medicine; that during an ordinary epidemic of scarlatina, influenza, or any other disease, an average of thirty cases fall under the observation and treatment of each: now if these twenty-five practitioners possess the requisite qualifications for reliable observations, with the zeal and industry to record their results, they would have seven hundred and fifty cases of one disease occurring in the same locality, and during the same season, for analysis and comparison. But let us suppose that Nashville is not more fortunate in this respect

than other cities, and that instead of her twenty-five practitioners being all competent and willing cultivators of therapeutics, there are only some two or three; then instead of seven hundred and fifty cases being brought into juxtaposition, there will only be from sixty to ninety—being a clear waste of over six hundred cases.

And how much greater must be the proportionate waste in smaller towns and in the country, where now and then but a single individual possesses the qualifications and will to make original research, and he, for the want of kindred spirits to coöperate with him, and from a knowledge of the impossibility of one season presenting more than a few cases of the same disease, fails to make record of the few cases he does witness.

4. A fourth cause of the comparative slow advancement of Therapeutics, consists in the *want of harmonious agreement among its cultivators as to the proper and legitimate intellectual mode of prosecuting their researches.*

From a very remote period in the history of medicine, two opposing ideas have occupied and divided the professional mind—as to whether practical medicine was, or could be reduced to, a rational system—or whether it was not purely an empirical art, founded alone upon experience.

Since the memorable reformation introduced into the cultivation of science and philosophy by Lord Bacon, the importance of facts obtained by observation and experiment, has gradually worked its way into every department of learning, until at the present time it seems fully recognized by all truly learned and philosophic minds.

I am not aware that there are any medical writers worthy of consideration, who at all deny the importance of clinical observation in therapeutic cultivation. There are many who make quite a wild and fanciful use of their reasoning faculties, (as there are others who make a bungling and aimless application of their powers of observation,) yet they admit the importance of clinical facts. I repeat that I am not aware that there are now any medical champions who oppose *á priori* reasoning to direct observation in the cultivation of practical medicine, and who hope to build up a system of rational medicine independent of the aid of experience and observation. There are those,

however, and I may say they constitute a majority of the great minds now successfully laboring in the field of practical medicine, who believe that our principles of practice are more rational than they once were; who believe that as the practitioner becomes more intimately acquainted with special and general anatomy, physiology, pathology, semiology, and the peculiar action of remedies, that he will gradually enlarge the domain of general therapeutics. In other words, that as the laws of vitality become better understood, under the influences of health and disease, and the action of remedial and prophylactic agents, our practice will become more and more rational.

With this temperate and laudable hope for the ultimate success of medicine as a rational art, there is no longer any reason why this subject should distract and divide the medical mind; and indeed it would cease to do so, but there are those on the other hand who keep up a continued fire against what they term the *rational system*; who declare themselves *partizans* to the school of empiricism; who assert that there can be no rational rules in therapeutics; who say they can give no other reason for using a particular remedy than that it has cured a disease similar to the one under treatment. In short, who affirm that all treatment in whole and in detail, must be based upon anterior clinical experience.

The contracted and exclusive doctrines of this school, in my humble judgment, is calculated greatly to retard the progress of therapeutics; and it is this consideration which I trust will plead my apology for stopping to examine on this occasion some of the chief arguments used to establish them. I have a double object in this examination: first, to show the evils of the system itself; secondly, to show the evils of *partyism* in medicine—that how minds qualified by nature and by education for the higher pursuits of philosophy, may be seduced into false and illogical reasoning by the deceptive influences of *partisan* bias.

I shall notice the arguments of only two of those champions of empiricism, they being the more generally circulated and read in this country. They both claim to settle the point definitely. I allude to the French medical historian, M. Renouard, and our distinguished countryman, the late Professor Bartlett.

M. Renouard in an appendix to his able History of Medicine,

discusses at length and with much ingenuity the merits of "pure empiricism;" yet the pith of his whole argument may be stated and disposed of in a very short space. The following is what he conceives to be the "fundamental and universal principle of therapeutics," that "*every kind of medicine which has cured one disease, must also cure analogous diseases.*", (page 615\*). He says "it possesses all of the clearness and infallibility of a mathematical problem."

But when he comes to make a "rational application of the universal axiom of therapeutics," he admits that "no practitioner during his whole life has met two identical morbid cases, and nature perhaps never produced them exactly alike. We must therefore be content with a greater or less similarity." (p. 616.) The author then points out the mode of determining the requisite similarity, as follows: "Here is the abridged table of the principle characteristics which constitutes to-day the diagnosis of diseases, and by which we are able to discern the different morbid species, or the homogeneousness of each of them: First, the circumstances anterior to the invasion of the disease, which comprise the predispositions or diathesis, the occasional determining causes, contagious infections, etc.; second, the anatomical seat of the disease, i. e., the designation of the organ or tissue principally affected, and sometimes the indication of a vitiated humor; third, the mode and degree of the alteration of the organs; fourth, the idiopathic and sympathetic functional derangements, their regular or irregular, continued or intermittent course; finally, the cadaveric lesions found in those who have succumbed to the same morbid species.

"We see by this enumeration of the principal objects which constitute the diagnosis of a disease, that in order to be able to fulfill this condition well, we must unite to a most precise knowledge of nosography and pathology, the lights of anatomy, physiology, chemical analysis, pathological anatomy, etc., etc.," (p. 617).

Now, after this enumeration of the principal objects of diagnosis, are you better satisfied than before of the *mathematical clearness* and *infallibility* of this universal principle of therapeutics? Has the author indicated the *degree* of resemblance

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\*I quote from Dr. Comegy's translation.

necessary to bring diseases under this rule of therapeutics? As all diseases resemble in some respects and differ in some others, how much should they resemble to be cured by the same remedy or remedies? Where is the uniform and practical exactitude of the principle? Let the author answer. On page 619 he continues, "We are therefore led by the natural connection of ideas, and the irresistible force of things, to make a choice among the symptoms which present themselves in the course of the disease. We are obliged to ask ourselves the question, which are among the pathological symptoms, those which are of the greatest importance, those which are less grave, and those which are of so little value that they may without inconvenience be entirely overlooked?"

In view now of the infinite varieties of the human mind in the observation and relative appreciation of phenomena, arising out of diversities of mental capacity, educational training, and preconceived notions, how many would agree in the classification of the symptoms? What one would call grave and important, another would regard as less grave, and such symptoms as some would overlook, others would regard worthy of consideration.

But suppose there could be no disagreement upon the relative importance of the various phenomena presented by disease, has the author informed the readers how many of the different orders of signs and symptoms should be present to constitute diseases analogous—or rather sufficiently analogous to be cured by the same remedy? Had the phrase *identical diseases* been substituted for *analogous diseases*, in the statement of this "universal principle of therapeutics," it would have lacked even then the qualification of *identical circumstances*, to have given it logical precision and mathematical clearness; but as there are no *identical* diseases, of course we have to be content with a greater or less degree of resemblance, without any possible rule of determining the degree of resemblance necessary to constitute diseases *therapeutically* analogous, except that they *are cured by the same remedy*. The manifest destiny then of the attempt to make such a logical statement of this "fundamental and universal principle of therapeutics," as to give it "all of the clearness and infallibility of a mathematical problem," is the following proposition :

*Every kind of medicine which has cured one disease, must also cure other diseases sufficiently analogous to be cured by it!*

But again, it will be perceived that the author here allows the right, even the necessity of reasoning *á priori*, from anatomical, pathological and physiological data—in other words, to theorize—in order to determine the relative value of symptoms. Then why deny that right in determining the relative value of remedies? If it be legitimate to judge of the importance of a morbid phenomena from the physiological and pathological relations of the organ or function which is the seat of the phenomenon, why is it not legitimate to determine the value of a remedy from the physiological and pathological relations of the organ, tissue or function primarily impressed by it? What difference is there in a morbid or unnatural phenomenon, as a diuresis, emesis, stupor, etc., arising in disease, and one superinduced by a medicine, that requires experience alone in the one case, and theory in the other, to determine its value or importance? If we have a right to say, in making out a diagnosis, that from the pathological relations of the stomach, constant nausea is an important symptom, have we not also the right to say in making up the treatment of a disease from the physiological relations of the stomach, a medicine that produces constant nausea is an important remedial agent? I must admit that I cannot see the difference of principle. I cannot see why we should depend alone upon experience in the once case, and not in the other. I do not comprehend why we may not reason as to the remedies, as the learned author allows us to do with regard to the symptoms, to-wit: "We are therefore led by the natural connection of ideas, and the irresistible force of things, to make a choice among the remedies (symptoms) which present themselves in the course of a disease. We are obliged to ask ourselves the question, which are among the remedies (pathological symptoms) those which are of the greatest importance, those which are less valuable (grave), and those which are of so little value that they may be entirely overlooked?"

Finally, how can the author declare "that we have endeavored to establish this proof (therapeutic) on a fixed, evident, and incontestable principle, not subject to the vicissitudes of pathological theories—and we have found that this principle may be

expressed as follows: that medication which has cured one disease, must cure equally all analogous diseases."

I say, how can he assert that this principle is independent of *pathological theories*, when he allows the necessity of *pathological theories* in determining analogous cases?

Our own countryman, Professor Bartlett, has not been more successful in his efforts to establish the claims of empiricism as the true and only basis of therapeutic cultivation, as I think I will be able to show.

I make the following extract from his work.\* He says, (p. 113) "*Therapeutics is not founded upon pathology. The former cannot be deduced from the latter. It rests wholly upon experience. It is, absolutely and exclusively, an empirical art.* There is but one philosophical or intelligible *indication*, and that is to remove disease, to mitigate its severity, or to abridge its duration; and this indication never grows out of any *á priori* reasoning, but reposes solely upon the basis of experience.

"It follows from what has been said in the foregoing pages, that the therapeutical action of the substances and agents of the *materia medica* is not to be inferred from their effects upon the body in a state of health. Their pathological relations are not to be deduced from their physiological relations. After having ascertained that the effects of tartrate of antimony or ipecacuanha taken into the stomach is to excite vomiting, we might to be sure, independent of experience, have been led to administer one of these articles for the purpose of removing from the stomach by the act of vomiting, any poisons or irritating substances taken into it. So in cases of disease attended with long continued vigilance, having ascertained the power of opium to produce sleep in a healthy condition of the system, we might be led by *á priori* reasoning, to use the same substance for the purpose of overcoming the morbid wakefulness. But even in these and in all analogous instances, excepting perhaps where the action of the article is to remove the cause of the disease, as in the case of offending matters in the stomach, just alluded to, or where the action of the article may be strictly chemical or mechanical, it is only by actual experience that we

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\* *An Essay on the Philosophy of Medical Science.*—By Elisha Bartlett, M. D., etc.

can ascertain the effects of the remedies upon the system laboring under disease.

"It does not necessarily follow, that because opium usually occasions sleep when taken into the healthy system, it will always remove the vigilance of disease. The philosophical reason of this is obvious. Therapeutics consists in the ascertained relations between the substances and agents of the *materia medica*, and *morbid* actions and conditions of the body, not between these substances and agents and the *healthy* actions and conditions of the body. And the philosophical reason is sustained by experience.

"There are many circumstances in which the morbid wakefulness attending upon disease is not removed or mitigated by opium, in whatever quantity it is administered. Look at delirium tremens. It is now very well settled that opium has but little effect in procuring sleep in this disease.     \*     \*     \*

"Is there any thing in the physiological relationships of cinchona, that could have led, without the teachings of direct experience, to a knowledge of its pathological relationships?

\*     \*     \*     Calomel when introduced in moderate quantities into the system in a state of health occasions severe local inflammation, attended with general febrile excitement. Is there any thing in this action of calomel which indicates the power of the same substance to arrest and control extensive and intense local inflammation?" etc., etc.

These extracts contain the most pointed and direct arguments of the writer upon this subject, and the fate of the system he is defending repose almost wholly upon its truth or falsehood. If he has succeeded in establishing the proposition that the actions of medicines in disease cannot be inferred from their action in health, then of course no anterior experience of the mode of action of a remedy on the healthy organism can be of service to us in making out the treatment of a disease; but on the other hand, if he has failed to make this proof, or if the converse of this proposition can be shown to be true, then the whole of his argument falls, and the consequent conclusion follows that we may have a rational view in administering a remedy prior to any experience of its effects upon any particular disease. In other words, that we may reason *a priori* in deter-

mining a course of treatment. It will also follow as a logical consequence, that as a general law, the pathological relationships of the organs and functions may be inferred or deduced from their physiological relationships, i. e., the organism *under disease* is not so different from the organism *in health*, as to render its general operations in the former condition independent of those laws which govern it in the latter state.

But how has the author succeeded? He says, "It does not necessarily follow that because opium usually occasions sleep when taken into the healthy system, it will always remove the vigilance of disease."

This proposition is clearly evasive in its statement. Why was the term *usually* used in the first member of the sentence, and *always* in the second? To have made the statement logical, it should have been thus:

*It does not necessarily follow that because opium ALWAYS occasions sleep when taken into the healthy system, it will ALWAYS remove the vigilance of disease; or else, it does not necessarily follow that, because opium USUALLY occasions sleep when taken into the healthy system it will USUALLY remove the vigilance of the disease.* The first corrected proposition, however, would not have done, as opium does not *always* occasion sleep in the healthy system; the second, of course, would have been fatal to the theory, (as opium does *usually* remove the vigilance of disease,) and the occasion for the "philosophical reason" to sustain it would have been done away with; and as to experience, that has long since established the general law that medicines do act upon the diseased organism in the same way as they do upon the healthy. Opium *usually* produces sleep in the healthy organism, it usually does the same in disease; ipecac, tartar emetic, sulphate of copper, etc., usually cause nausea and vomiting in health—they usually act in the same way in disease; castor oil, jalap, aloes, rhubarb, etc., usually increase the action of the bowels in health—they usually produce the same effect in disease; calomel taken in health ordinarily excites the functional activity of the liver and other glands, and when its use is protracted causes salivation—it has this identical effect ordinarily in disease.

But this worthy expounder of the doctrines of empiricism,

strange to say, forgetting these obvious and well settled facts, asserts that the "philosophical reason (why the action of medicines in disease cannot be inferred from their action in health) is sustained by experience!" and he offers as examples the uncertainty of opium in *delirium tremens*, the power of cinchona over intermittent fever, and the control of calomel over acute inflammation.

He has searched out a few apparent exceptions to a general rule, to establish a converse law.

But more strange yet, just here where experience is so abundant and conclusive, and *à priori* reasoning so powerless, the author introduces his "philosophical reason" as the *principle* weapon of argument, and appeals to experience only as secondary support. But had not the learned author of the Philosophy of Medical Science been swayed by party bias; had he not declared in advance his adhesion to a school, but investigated the subject with that clear perception of the truth which characterizes all his other writings, he could not have but seen this singular perversion of fact. He would have seen that the examples here given to establish a general law by *à priori* reasoning, were in truth but exceptions to a converse law already established by long and abundant experience; he could not have but expressed himself something after this manner: That as a general rule, medicines act upon the sick as upon the healthy; but that there are some exceptions to this rule; for example, if the organ or tissue upon which a medicine primarily acts become insensible or paralyzed, of course the medicine would fail to act in such a case as it would do in health, or in other forms of disease—if the peristaltic action of the intestines become paralyzed by inflammation of the muscular coat or otherwise, purgatives of course could not act—if disease has rendered the stomach insensible, emetics would fail to act, just as light refuses to impress images upon an amaurotic eye. But suppose the arguments here used were allowed to be strictly logical, and that it was demonstrated that the knowledge of the action of a remedy in health could afford no grounds for inference of its action in disease, then could not the argument be retorted to prove that a knowledge of the action of a remedy in one case of disease could afford us no reasonable ground of

inference of its action in any other case whatever, and of no value in the treatment of disease?

Let us see. I will reason as Professor Bartlett has here reasoned. "It does not necessarily follow that because calomel abridges the duration of acute inflammation in one subject, that therefore it will abridge the duration of inflammation in all other subjects, or even any other subject. The philosophical reason of this is obvious. Clinical experience consists in the ascertained relations between the substances and agents of the *materia medica*, and morbid actions transpiring in subjects *under the observer's eye*, not between these substances and agents and the morbid actions of any other subject. And the philosophical reason is supported by experience, for there are many circumstances which render opium powerless in procuring sleep in one subject, though it had succeeded in procuring sleep in another subject with a similar disease. Look at calomel in acute inflammation, say pneumonia, its curative effects are marked and positive in one case, whilst in another, in every sensible way similar, it wholly fails. Indeed, is it not an every day occurrence that one man's experience contradicts that of another."

Need I now stop to show the evil effects of partyism in medicine? Need I introduce any argument drawn from the examples of medical logic just cited to show how narrow and one-sided are the views, and how fallacious may be the arguments of our brightest intellects when swayed by partisan bias? Or need I elaborate an argument to show the evil tendencies of this system of empiricism in itself? Need I make a special point to show that in all investigations after truth the greater the variety and number of *means* and *powers* brought into requisition, the more thoroughly and completley will the subjects be analyzed and exposed. That if therapeutics has made slow and doubtful progress under the control of experience and observation, *along with the aid of sound deduction*, it will hardly progress any faster under the control of *experience and observation alone*? Need I detain you to argue the point, that if there can be no *a priori* reasoning upon the action of remedies in disease, there can be no such thing as general therapeutic rules; thus directing the professional mind away from the investigation and con-

temptation of those great laws of vitality, under the various influences of climates, seasons, aliments, poisons, medicines, etc., in the final solution of which we hope ultimately to place practical medicine upon a rational basis,—to the search after specifics, and the construction of nicely proportioned combinations of drugs for the cure of particular diseases? I feel assured that I need not elaborate these questions, but be allowed to pass on to the notice of another cause that hinders, comparatively, the progress of therapeutics.

5. The fifth and last cause to which I invite your attention, consists in *the want of that unity of purpose, combination of power, and concert of action found in the societies formed with a view to the promotion of special enterprises.*

I will not enter upon the consideration of the importance of organization to the successful cultivation of science and art—that subject is fully understood and appreciated by every one. I will simply call attention to the fact, that whilst nearly every branch of medical learning is being cultivated by the labors of societies directed to special investigations, therapeutics, the most important of all of them, has mainly been indebted for its advancement to individual labors. If there are any societies in Europe or America formed for the special end of advancing the art of therapeutics, I am not apprised of it. Physiology, pathology, pathological anatomy, chemistry, the art of surgery, and even the history, causes, etc., of epidemic diseases, have all of them organizations destined for the special cultivation of those several departments.

It is true there are medical societies in every country where medicine is pursued as a learned profession; but they are associations for the advancement of medical science and art in general, and so far from therapeutic advancement being their immediate object, that department seems to receive the smallest share of attention. This seems the more surprising as it is allowed that the perfection of the medical art is the prime object of the associate sciences.

Having pointed out, in as succinct a manner as the importance of the subject would allow, the most obvious causes of the comparative slow advancement of therapeutics, I now propose briefly to suggest

*Those means which seem best calculated to remove them.*

1. The first cause to which I adverted as tending to retard therapeutic cultivation consists in the extent of scientific field which it covers, the complication and widely dissimilar nature of the facts and principles involved, requiring such a vast amount of labor, as well as varied and multitudinous qualifications for successful observation, experiment and deduction.

As this cause is found in the nature of things, of course it cannot be removed; yet it seems that something may be done to obviate its force. In the eager pursuit after new remedies and new inventions, the medical mind appears to have lost sight of the vast increase of medical facts and appliances, and has omitted to grasp and fully realize the necessity of proportioning the term of medical pupilage to the increased growth of medical science.

When it is remembered that the relationships of the animal organism to every thing around us is such that every branch of natural science contributes more or less to the fund of medical lore, and that those branches are daily reaching out into new and more extended fields of conquest—I say, when these things are remembered, it cannot be surprising that the pupil, who serves only the term of pupilage that was required when medical science covered less than half the ground it now occupies, must go into the practical duties of medicine ill prepared to make good use of the phenomena that pass before him in the sick-room.

The first requirement of a cultivator of therapeutics is that of a knowledge and skill in the application of the rules of diagnosis. But look for a moment at the long stride that this branch of medicine has made within the last forty years. Within that period auscultation as a means of diagnosis has gradually evolved its wonderful powers—first over the diseases of the lungs, heart and large vessels, and secondly over the diseases of the abdominal viscera, as well as the physiological and pathological states of the gravid uterus and its contents—when at the present time this important contribution embraces a scope of learning, and imposes an amount of practical training sufficient to consume almost one-half of the usual term of medical pupilage.

Within the same period chemico-microscopic pathology has received such acquisitions as to develop the necessity of other and still more elaborate means of physical diagnosis—means exacting yet more extensive acquaintance with general science for their comprehension, and more varied and accurate tact for their application.

But this is not all. Chemistry and the microscope have conducted the medical enquirer into other and before unknown fields of research, swelling beyond all anticipation by the rich treasures they reveal, the dimensions of anatomy, physiology, and pathology, thereby greatly increasing the amount of learning necessary to render the practitioner competent to make those minute enquiries into the anatomical and pathological characteristics of disease that give interest and authority to his observations.

Without mentioning the various important contributions to the history, pathology, etc., of particular diseases in the form of monographs, enough, I trust, has been said to show the immense disproportion between the term of medical pupilage, (particularly in our own country,) and the increased amount of labor that the interest of medicine requires of the student.

Of course the limits to which I am necessarily confined do not allow of any discussion of the subject in detail—as to whether the increase of term should embrace the private or public pupilage, or both—the means by which this is to be accomplished, etc., etc.; nor does it enter into the design of this discourse to do so, my object at present being to notice some of the most salient points of defect in therapeutic cultivation, and to suggest only general steps for its reformation.

2. The second cause to which I have invited your attention, *consists in the scarcity of available material for experimentation.*

In order to the most speedy and successful cultivation of therapeutics, three orders of experimentation are necessary; first, to ascertain the action of medicines on the healthy organism; secondly, to ascertain the natural course, development and termination of diseases; and thirdly, to ascertain the mode of action of medicines in disease.

The first order—to ascertain the action of medicines upon the healthy organism—of course can only be conducted with

satisfactory results upon the experimenter himself. It is well known that such experiments have, from time to time, been undertaken, and in some cases heroically persisted in; yet they have not been sufficiently general, too few practitioners have sacrificed their ease and comfort for such dear bought knowledge; such experimental knowledge, however, is all but essential to a great practitioner. If physicians would occasionally swallow the medicines they are in the habit of giving to their patients, their *sensible* effects upon the stomach or brain would soon teach them that much of their treatment is calculated only to embarrass the natural processes of cure.

This order of experimentation requires to be conducted on a more extensive scale, and with more method and pointed interrogation, than has hitherto been done. The new lights thrown by recent researches upon the physiological relations of the organs, functions and tissues, require that the action of the remedies daily used to correct their pathological changes, should be reconsidered under the new lights of physiology and pathology. Instead of swelling the dimensions of the *materia medica* with new remedies of secondary consideration, it would be far better that we perfect our knowledge of old and important ones, by investigating their various influences under the largest possible number of circumstances. Opium, mercury, antimony, quinia, iron, colchicum, etc., etc., are remedial agents that long and abundant experience has demonstrated to exert powerful influences over disease; yet their modes of impressing the organism are far from being understood. How do they affect the mucous coats of the stomach? How the nervous, vascular and glandular systems? How the functions of nutrition, calorification, sanguification, innervation, etc., etc.? These are questions that must be answered, and their solution can only be hoped for in patient and laborious experiments upon the healthy organism.

The second order of experimentation—*to ascertain the natural development, course and termination of disease*—is no less important to the progress of therapeutics.

It must be evident to every sound mind, that in order to profit by our experience in the treatment of disease, we must be in a condition to determine between the natural processes of

cure or of decline, and those processes as superinduced by artificial agents. There is scarcely a remedy made use of but acts in imitation of nature. Blood-letting and blisters find their archetypes in spontaneous haemorrhage and those sero-plastic exudations that occur in some stage of almost every acute inflammation; emetics, cathartics, diuretics, diaphoretics, etc., etc., have each and all of them effects in every way similar to those arising spontaneously in disease. Unless we know then by observation, how nature proceeds in her various steps to terminate a diseased action, unless we can trace the causal connection of those various spontaneous phenomena, with the different modifications and terminations of disease, how can we know what actions should be modified, arrested, or new action induced? And how can we distinguish the changes and terminations spontaneously arising from those superinduced by our remedies? How can we know but that in many cases instead of aiding the natural processes of cure, we embarrass, or even defeat them by our interference? Who is it that knows the relative proportion of cases of pneumonia, pleuritis, or cholera that would recover when left alone to nature? And who is it that knows the average duration of those diseases when not treated by medicines? These questions can only be solved by united experience and observation. But I have already adverted to the fact that it is contrary to the moral status of the profession to experiment upon man. The declaration, however, requires some qualification, for every practitioner must at times make trials, the result of which cannot be foreseen—in other words must experiment. Such procedure is allowable and even necessary, yet the object of such trials must always be the good of the subject upon which the trial is made, and not for any ulterior good. No one has a right to hazard the life or sacrifice the comfort of an individual, placed under his treatment for the good of others. There is no species of practice, whether in hospitals, charities, or even prisons and houses of correction, but implies an obligation upon the part of the physician to use every means at hand to relieve the pain, and cure the disease of his patient. But as every disease, however new and extraordinary, presents an aggregate of symptoms that suggests to the mind of the practitioner a course

of treatment offering more or less probability of success, therefore some treatment becomes imperative, and thus all hope of aid from this order of experimentation seems to disappear. But from a close and philosophic view of the present unsettled state of therapeutic rules, may we not yet hope, by a united effort of enlightened practitioners, made in the right direction, with a lively sense of professional obligation and a clear view of the wants of medicine and the difficulties to be encountered, that much yet may be accomplished in the ascertainment of the natural progress and termination of disease.

The history of medicine embracing the results of various systems of practice, both *legitimate* and *illegitimate*, goes to show that therapeutical treatment does not alter the rate of mortality, or change the manner and period of termination of disease in near so marked a manner as is usually supposed. In other words, that the animal organism has so strong a tendency to terminate disease in its own *manner* and *time*, that injudicious practice does not kill or protract disease, nor prudent and enlightened practice save from death or abridge the duration of disease near so often as is generally thought. So strikingly true is this observation, that all over the civilized world the most enlightened practitioners have greatly simplified their practice. Without any great reformer to take the lead, they have, simultaneously as it were, been awakened to the truth that nature had better be followed and assisted than preceded and controlled. The medical mind then seems prepared for an important step in therapeutic cultivation—that of the investigation of the natural development, progress and termination of disease. It seems prepared, by a proper appreciation of the yet doubtful and uncertain state of therapeutic rules, the dangers of over, or injudicious medication, and the sufficiency of nature in a large majority of cases to accomplish a cure, I say the medical mind seems now prepared to enter upon a more minute and vigorous study of the natural processes of morbid actions, without the violation of professional duty, than has hitherto been done. From all of the lights now before us, it is clear that a large majority of the cases in almost every class of curable diseases requires a treatment so mild and simple as to disturb or alter but in a slight degree the natural processes of

cure: and although the wants of therapeutic cultivation cannot thus be fully attained, yet by a faithful observation of the alterations and modifications of disease, and a careful scrutiny of the effects of our remedies and their relations to those alterations and modifications, in the lapse of time, when a large number of such observations shall have accumulated for analysis and comparison, such an approximation to truth may be attained as will in a great measure give precision and certainty to the therapeutic art.

The third order of experimentation—*to ascertain the mode of action of medicines in disease*—was instituted at the commencement of the medical art, and has been pursued down to the present time; but from the limited knowledge of the action of remedies on the healthy organism, and the imperfect and often fanciful views of the physiological and pathological relations of the various organs, tissues and functions, together with the aimless and bungling manner of conducting those experiments, therapeutics has as yet derived comparatively but little permanent advantage from them. No one can review the history of therapeutic progress without being struck with the vast amount of clinical experience rendered worthless from the loose and imperfect manner in which the observations were made, the vague and unsatisfactory detail of the sequence of phenomena, and often the mixing up of so many medicines into one compound as to render the analysis of phenomena and their respective causes all but impossible. Out of the endless number of reported cases and their treatment that fills the journals of the present time, how few there are worthy of consideration, or calculated to advance the interest of medicine! There seems to be a prevailing idea that every case of illness would prove fatal without some treatment, and therefore the fact that the case or cases recover under a specific line of treatment, settles the efficacy of the treatment. No neat distinction is made between those curative modifications arising spontaneously and those made by the action of medicines. Indeed all changes that occur for the better are credited to the remedies, and all changes for the worse are the works of nature; and thus it is *clinical experience* is invoked to establish the efficacy of medicines in certain diseases, wholly opposite in their modes of

action. But how can this state of things be remedied so long as the practitioner interests himself alone with the *curative* effects of his remedies—whilst he eagerly turns all of the curative phenomena to the account of his treatment, and neglects to notice the manner in which nature operates to terminate diseased actions? It is evident that the natural development, progress, and termination of diseases must be more thoroughly studied and better understood before much progress can be made in therapeutic cultivation. It is also evident that the various actions of remedies upon the healthy organism, under the many circumstances of age, sex, temperament, etc., etc., must be in like manner better known to enable us, with some rational grounds of safety, to make trials of them in disease.

3. The third cause noticed as tending to retard the progress of therapeutics, is that of *the dependence of therapeutic cultivation upon the contingency of popular choice of the cultivators*.

It can hardly ever be in the power of the medical profession, either as individuals or as an organized body, to correct directly this evil. The popular mind itself will have to be greatly enlightened upon medical subjects before it can make that distinction amongst medical men which is necessary to enable it to place the professional management of the sick in the hands of those who are best qualified for such responsible trust. It is only possible that we may indirectly contribute to a more correct popular view of the nature of the learning and gravity of the responsibilities of the medical profession, and a more just sense of the requisite qualifications in its practitioners, by instituting some acceptable method of enlightening the public mind upon medical subjects. The occasion, however, will not allow of any discussion of this topic.

4. The fourth cause presented to your minds, consists in *the want of harmonious agreement among the cultivators of medicine as to the proper and legitimate intellectual mode of prosecuting their researches*.

I have endeavored to show under this head that there was no good reason for disagreement as to the intellectual method of prosecuting medical researches; that all men acknowledged the necessity of observation and experiment in the cultivation of therapeutics, as well as every other science or art. Because

there are some who have more imagination than others, and are satisfied with fewer and less positive data for deduction, there is no reason to charge them with being *adverse* to experience and observation, and because they make a failure in their speculations, to denounce the legitimacy of *à priori* reasoning altogether. The same method of reasoning would exclude observation and experiment, since those methods are often exercised in a manner quite objectionable, and wholly fail to establish what they claim to do. I have moreover endeavored to show that the exclusive claims of "pure empiricism," as defined by Professor Bartlett and Dr. Reanouard, are untenable and absurd. Medicine is not and cannot be purely an empirical art. Emergencies will arise every day when the practitioner must call into requisition his stock of knowledge of physiology, general pathology and therapeutics, as a guide to his further procedure. Phenomena are constantly arising, obliging him to reason *à priori* with regard to their causes, influences, and the probable result of the application of certain remedies whose mode of action is presumed to be known. Such exercise of reason no one can object to, indeed every one must see and allow its necessity. Then if such reasoning be allowed, and the result be clear and satisfactory to one's-self, why deny him the right of offering it to others? In other words, why deny him the right to *theorize*? Indeed there is always less harm to be derived from a false theory than from false experience. The former appeals to your reason while the latter claims your faith; the one may be discarded as an error of mind, the other must be entertained though you do not believe it; hence false theories are definitely disposed of, whilst false experience stands always in the way. Therapeutics is this day suffering more from false experience than from false theories.

The wants of practical medicine are many and varied, and require the aid of every means of investigation—facts must be discovered and general laws must be deduced—every educated and well organized mind may find labor congenial to its taste and suitable to its capabilities. In the language of Lord Bacon, "let there exist then (and may it be of advantage to both) two sources and two distributions of learning, and in like manner two tribes, and, as it were, kindred families of contemplators

or philosophers, without any hostility or alienation between them, but rather allied and united by mutual assistance."

5. The fifth and last cause to which I invite your attention, *consists in the want of that unity of purpose, combination of power, and concert of action found in societies formed with a view to the promotion of special enterprises.*

We have too many examples of the fruitful results of the labors of organized bodies, to require any argument to prove the benefits likely to result from the formation of societies for the special purpose of promoting the interest of therapeutics. It is presumed then that the simple mentioning of the fact—the awakening of the medical mind to a realization of the neglect of this important branch of medicine—will be enough to arouse medical practitioners to something like emulative industry in the cultivation of their own special department. It will not be contended here that the formation of societies for the sole purpose of therapeutic cultivation, is practicable over a wide district of country. It is granted that they are only practicable in large cities, where medical men can meet often enough to keep up a lively interest in the subjects of their researches, where a sufficient number in a limited district can be found for efficient organization, and where daily coöperation can be successfully carried on. I will not assert that we can form a National or State society for such special purpose in medicine. The inconvenience of meeting is too great for more than one or two conventions in twelve months, and then we must embrace in our labors all of the great interests of medicine. But the same difficulties do not apply to New York, Philadelphia, London, Paris, and other great cities. *There* may be found ample materials for the formation of societies for the special promotion of therapeutics. Indeed there are constantly being formed in the great cities of Europe, new societies for the advancement of the various interests of medicine, nor does such increase in numbers seem to weaken the efforts or lessen the aggregate of results, but rather, by stimulating a laudable rivalry, to promote the interest of all. It should not be denied, however, but that our State and National organizations have too much neglected the cultivation of this important branch of medicine; that in apportioning out the labors of the various

committees, therapeutics has not been given that prominence that its transcendent practical importance merits.

The great step at reformation in this direction, however, must be taken in the metropolitan cities—in the great centres of medical learning—and no one can foresee the immense impetus that would thus be given to the advancement of the therapeutic art. Indeed such a step may be regarded as the first and most necessary towards a great medical reformation; for in such societies would all of the errors of cultivation find a correction, and the various wants be considered and provided for. We have only to consider for a moment the legitimate labors that would devolve upon such bodies, to comprehend the variety and magnitude of subjects to be investigated, and the versatility of mental powers and educational advantages that would be called into active requisition.

Such societies would engage in the accumulation of clinical facts; experimentation with medicine upon the healthy organism; the investigation of diseases in reference to their natural development, progress and termination; the progress and termination of diseases under the influence of medicines; the examination of the probable bearing upon therapeutics of the daily discoveries in physiology, pathology, organic chemistry, histology, etc., etc.; the verification or disproval of pretended discoveries; and the critical examination of such doctrinal points as relate to therapeutic laws.

These would form some of the labors of such societies, and they would tend to give unity to practical medicine; they would in time result in a great source of authority that would command the confidence of the learned and conscientious, and hold the ignorant and rash pretender in submissive awe.



